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# Four Score and Seven Years from Now: The Date/Delay Effect in Temporal Discounting

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We describe a new anomaly in intertemporal choice—the “date/delay effect”: discount rates that are imputed when time is described using calendar dates (e.g., on October 17) are markedly lower than those revealed when future outcomes are described in terms of the corresponding delay (e.g., in six months). Date descriptions not only reduce discount rates, but also affect the implied *shape* of the discount function: When inferred from intertemporal choices between options referenced by calendar dates, the discount function appears markedly less hyperbolic. We discuss potential psychological bases of the date/delay effect, its implications, and other modes of temporal reference.

*Key words:* intertemporal choice; framing effects; decision making

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## Introduction

In his Gettysburg Address, Abraham Lincoln used the powerful phrase “Four score and seven years ago” to refer to the time that had elapsed since the founding of his nation. It is unlikely that Lincoln chose this phrase casually, or that he would have been equally satisfied with “about 90 years ago,” or “in 1776.” He knew that the effect of his speech depended as much on the words he chose as on what they referred to.<sup>1</sup>

In a more mundane context, experimenters investigating intertemporal choice must also decide how to describe time to their respondents. To illustrate, we may want to know the present value that someone places on £100 to be received in 18 months. We can refer to that temporal interval using different units

of delay (e.g., 550 days, 1.5 years), combinations of these units (e.g., one year and six months), or calendar dates (e.g., on July 5, 2006). Even a casual reading of the literature will reveal that different experimenters choose different descriptions and that they rarely if ever speculate about the implications of their choices. This implies a belief that the way time is described does not matter.

This assumption is inconsistent with the wealth of evidence demonstrating “framing” effects in other contexts. For example, identical outcomes lead to risk seeking when they are described as losses relative to an arbitrary reference point, and risk aversion when they are described as gains (Kahneman and Tversky 1983); the decision weight placed on unitary quantities (such as probability and time) increases when they are decomposed into formally identical subsidiary components (Read 2001, Starmer and Sugden 1993); and choices between gambles can even be influenced by whether the payoffs are listed in columns or rows (Harless 1992). These findings show that we cannot assume the impact of temporal distance is independent of how time is described.

<sup>1</sup> If not Lincoln then perhaps his public relations advisor, a role adopted by Bob Newhart in one of his monologues:

You changed four score and seven to...to 87?!...but, Abe, that's meant to be a grabber. Abe, we test marketed that in Erie and they went out of their minds. Trust me, [if we use “87”] it's sort of like Marc Antony saying, “Friends, Romans, Countrymen, I've got something I wanna tell ya!” See what I mean, Abe?

In this paper, we focus explicitly on the effect of describing time using calendar dates versus units of delay. We were inspired by a passage from Robert Strotz's (1955) seminal paper on intertemporal choice:

The relative weight which a person may assign to the satisfaction of a future act of consumption (the manner of discounting) may depend on either or both of two things: (1) the *time distance* of the future date from the present moment [what we call the *delay*], or (2) the *calendar date* of the future act of consumption. (pp. 167–168)

This passage summarises a general model of time discounting in which the present value of a delayed outcome is the product of two factors: its *future value* (the value it will have when it is received), and the *discount factor* (the weight currently accorded to this future value, which generally diminishes as function of delay).

The passage also lends itself to an additional interpretation. It points out that there are two ways to describe the moments at which outcomes will occur: as a delay from the present, or as a specific point in time identified by a calendar date. It also suggests to us that these descriptions correspond to different psychological processes. Strotz may have considered this possibility, as the above passage continues:

To the extent that time-distance is important, I may assign a different (and probably higher) weight to September 26 as it draws nigh; if only the calendar date is important, the weight will not change as that date approaches. (p. 168)

One interpretation of this passage is that the degree to which future outcomes are discounted may depend on whether we direct our attention primarily toward the delay (time-distance) or the date (the future moment when the outcome occurs), and that we will discount more if we focus on the delay than on the date.

Following Strotz's suggestions, we propose the *date/delay* hypothesis—that the valuation of a future outcome will depend on whether its timing is described in terms of delays or calendar dates. Specifically, using units of delay will draw our attention to how long we will wait, whereas using calendar dates will draw it to the moment the outcome will occur, and on the value it will have at that time. Therefore, people will be more patient (they will discount less) when time is described as a calendar date than when it is described in terms of units of delay.

In this paper, we describe five tests of the date/delay effect and demonstrate that it is a replicable and robust phenomenon. Experiments 1 and 2 show the date/delay effect for two elicitation procedures widely used in studies of decision making—choice and matching. Experiment 3 demonstrates that the

date/delay effect occurs even when real money is at stake. Experiment 4 investigates what happens when both descriptions are presented concurrently and reveals that the delay description appears to take precedence. Finally, Experiment 5 highlights another important difference between the two possible frames: Hyperbolic discounting occurs for delay, but not date, descriptions. In our discussion, we consider possible causes for the date/delay effect, and sketch out some of its implications.

## Experiment 1

In Experiment 1, participants chose between smaller-sooner (SS) and larger-later (LL) options. The interval separating the outcomes was identical though it was described in different ways, using either calendar *Dates*, or *Delays* (a number of *Months* or *Weeks*).<sup>2</sup>

## Method

Ninety students from the London School of Economics were approached in the courtyard or library, randomly assigned to the Date, Month, or Week conditions, and checked the box of the option they preferred in four questions of the following type:

	Option 1	Option 2
You receive	£370	£450
When	September 26, 2003	June 25, 2004
Your choice	<input type="checkbox"/>	<input type="checkbox"/>

Half answered the four questions in the order given in Table 1, and half answered them in the reverse order. Four incomplete questionnaires were excluded from the analysis.

## Analyses

The dependent measure was the number of times each participant chose the larger-later reward (LL). As Table 2 shows, there was a strong date/delay effect: Twice as many people chose LL in the Date condition than in the Week or Month conditions. Differences across conditions were tested formally with Kruskal-Wallis tests. An overall test confirmed a significant difference between the conditions ( $\chi^2(2) = 21.1$ ,

<sup>2</sup> The dates in this experiment, and in all subsequent ones, were chosen to ensure that our test of the date/delay hypothesis was conservative. Experiment 1 was conducted during the first weeks in June 2003, which meant that each date-interval began and ended approximately two weeks earlier than the delay-interval. If one assumes exponential discounting, this should make no difference because the discount rate is constant across time. If one assumes hyperbolic discounting, this procedure is conservative, because a hyperbolic discount function predicts *greater* discounting for events that are closer in time, and would therefore predict *more* choices of SS in the Date condition—the opposite of the date/delay effect.

**Table 1** Amounts and Times Used as Stimuli in Experiments 1–4

Question:	A		B		C		D	
	SS	LL	SS	LL	SS	LL	SS	LL
<b>Experiment 1</b>								
Amount	£370	£450	£520	£740	£770	£1,480	£900	£1,200
Date	Sept 26, 2003	June 25, 2004	July 25, 2003	Nov 26, 2004	Nov 28, 2003	May 26, 2006	Aug 29, 2003	Sept 24, 2004
Delay (months)	4	13	2	18	6	36	3	16
Delay (weeks)	17	56	9	78	26	156	13	65
<b>Experiment 2</b>								
Amount	£370	£450	£520	£740	£770	£1,480	£900	£1,200
Date	Oct 31, 2003	July 7, 2004	Aug 29, 2003	Dec 31, 2004	Dec 26, 2003	June 30, 2006	Sept 26, 2003	Oct 29, 2004
Delay (months)	4	13	2	18	6	36	3	16
<b>Experiment 3</b>								
Amount	£37	£46	£52	£82	£77	£128	£90	£138
Date	Mar 25, 2005	Dec 30, 2005	Jan 28, 2005	May 26, 2006	May 27, 2005	Oct 27, 2006	Feb 25, 2005	Mar 24, 2006
Delay (months)	4	13	2	18	6	23	3	16
<b>Experiment 4</b>								
Amount	£900	£1,200	£750	£2,000	£520	£780	£465	£870
Date	Feb 27, 2004	June 24, 2005	April 30, 2004	Oct 31, 2008	Dec 26, 2003	Oct 28, 2005	Mar 26, 2004	Feb 23, 2007
Delay (months)	4	20	6	60	2	24	5	40

$p < 0.0001$ ). Separate tests confirmed that the Date condition differed significantly from both the Month ( $\chi^2(1) = 11.7$ ,  $p = 0.001$ ) and Week ( $\chi^2(1) = 17.7$ ,  $p < 0.0001$ ) conditions, while the Month and Week conditions did not differ from each other ( $\chi^2(1) = 1.2$ ,  $p = 0.273$ ).

## Experiment 2

In Experiment 1, preferences were elicited using choice. In Experiment 2 we tested whether the date/delay effect is robust with respect to response mode, by observing whether it occurs for *matching* as well as choice. In matching the respondent provides a missing attribute value that will make two options subjectively equivalent, as in the following (where  $t_{LL}$  is the time at which the larger-later outcome ( $x_{LL}$ ) will be received, and  $t_{SS}$  is the time when the smaller-sooner outcome ( $x_{SS}$ ) will be received):

- \$370 in 17 weeks is equal to \$450 in \_\_\_\_ weeks (matching on  $t_{LL}$ )

- \$370 in \_\_\_\_ weeks is equal to \$450 in 56 weeks (matching on  $t_{SS}$ )
- \$370 in 17 weeks is equal to \$\_\_\_\_ in 56 weeks (matching on  $x_{LL}$ )
- \$\_\_\_\_ in 17 weeks is equal to \$450 in 56 weeks (matching on  $x_{SS}$ )

## Method

Participants included students from the London School of Economics (LSE) and visitors to a local business centre (Canary Wharf); 160 completed the questionnaire. As described above, subjects equated pairs of delayed outcomes by filling in a missing attribute value. For half of the participants, time was referred to as a calendar date, and for half it was referred to as a delay in months. Respondents were first shown the following practice question:

	Option 1	Option 2
You receive	£ ____ A ____	£500
When	August 10, 2003	November 24, 2003

They were then told “we want you to fill in the blank to make both options equal to you. In the example above you should state what value for A would make getting A on August 10, 2003, *just as good* as getting £500 on November 24, 2003.”

After answering this practice question, each participant made a matching response to four different questions (as described in Table 1), with a different attribute left blank for each question (i.e., each respondent matched once on  $t_{LL}$ ,  $t_{SS}$ ,  $x_{LL}$ , and  $x_{SS}$ ). Thirty-two forms were constructed by crossing two time descriptions (Date versus Delay) with the four

**Table 2** Experiment 1

	Description		
	Date	Delay (months)	Delay (weeks)
A (%)	60	29	14
B (%)	63	18	7
C (%)	60	43	32
D (%)	53	18	21
Mean (%)	59	27	19
N	30	28	28

*Note.* Percent choices of the larger-later (LL) amount given different descriptions of time.

attributes left blank ( $t_{LL}$ ,  $t_{SS}$ ,  $x_{LL}$ , and  $x_{SS}$ ), and with four question orders (ABCD, BCDA, CDAB, or DABC). For each question order, there were four groups based on which attribute was blank for which question. These are shown below. If a participant was in Group I, for instance, they filled in the  $x_{LL}$  blank for question A, the  $t_{LL}$  blank for question B and so on. Each of the four groups was divided into the four orders, yielding 16 forms in all. And each of these 16 forms were constructed twice, once for the date and once for the delay condition.

	A	B	C	D
I	$x_{LL}$	$t_{LL}$	$x_{SS}$	$t_{SS}$
II	$t_{LL}$	$x_{SS}$	$t_{SS}$	$x_{LL}$
III	$x_{SS}$	$t_{SS}$	$x_{LL}$	$t_{LL}$
IV	$t_{SS}$	$x_{LL}$	$t_{LL}$	$x_{SS}$

### Analysis

The dependent measure is the choice implied by each response if it had been a choice between an item with all the values from Table 1 filled in (for a discussion of choice versus implied choice, see Tversky et al. 1988). To illustrate, suppose that  $x_{LL}$  was missing in question A and the respondent filled in a value of £470. *Indifference* between £370 on October 31, 2003, and £470 on July 7, 2004, implies a *preference* for £370 on October 31, 2003, over £450 on July 7, 2004 (the values from Table 1).

Table 3 reports the percentage of participants who made implicit choices of *LL*, broken down either by question (A, B, C, D) or matching attribute ( $t_{LL}$ ,  $t_{SS}$ ,  $x_{LL}$ ,  $x_{SS}$ ). There was a clear date/delay effect, with more implicit choices of *LL* in the Date than in the Delay condition. We first tested this with a Kruskal-Wallis test comparing the total implied choices of *LL*

**Table 3 Experiment 2**

	Date (%)	Delay (%)
Broken down by question		
A	37	29
B	44	18
C	51	23
D	58	26
Broken down by response type		
$t_{SS}$	47	32
$t_{LL}$	33	15
$x_{SS}$	59	29
$x_{LL}$	51	19
Mean	48	24

*Note.* "Implied" choice—proportion of respondents whose matching responses implied a choice of larger-later (*LL*) if all cells had been filled in with values from Table 1.

in the Date and Delay conditions ( $\chi^2(1) = 17.8$ ,  $p < 0.0001$ ). Separate  $\chi^2$  analyses for each attribute confirmed the robustness of this result. There were more choices of *LL* when matching on  $t_{LL}$  ( $\chi^2(1) = 6.7$ ,  $p = 0.01$ ),  $x_{SS}$  ( $\chi^2(1) = 13.8$ ,  $p < 0.0001$ ), and  $x_{LL}$  ( $\chi^2(1) = 16.8$ ,  $p < 0.0001$ ), and  $t_{SS}$  ( $\chi^2(1) = 3.8$ ,  $p = 0.05$ ). Thus, the date/delay effect is found for matching as well as choice.

### Experiment 3

The choices respondents made in Experiments 1 and 2 were hypothetical. Although there is no evidence that hypothetical and real rewards are discounted differently (Frederick et al. 2003, Johnson and Bickel 2002), we conducted a modified version of Experiment 1 that included a random lottery incentive (e.g., Starmer and Sugden 1991) to ensure that the date/delay effect from Experiment 1 could not be attributed to respondents' failure to consider their options carefully. Sixty respondents, all LSE students recruited at the university library, answered four questions of the type asked in the previous experiment (see Table 1).

After completing a sample question, respondents were informed that two participants would be selected and paid according to their choices:

Two participants will receive one of their choices for real. They will receive a bank transfer for the chosen amount at the specified time. For instance, if your name was chosen for the above choice, and you chose Option 1, you would receive £40 on July 29, 2005 [in seven months in the delay condition], while if you chose Option 2, you would receive £55 on September 30, 2006 [in 22 months in the Delay condition].

Your email address: \_\_\_\_ (so we can contact you if you are chosen to receive a payment)

As Table 4 reveals, even when real money was at stake, the date/delay effect was substantial. Respondents chose the *LL* option far more often when time was referred to using calendar dates—a difference that was highly significant according to a Kruskal-Wallis test ( $\chi^2(1) = 19.1$ ,  $p < 0.0001$ ).

**Table 4 Experiment 3**

Question	Description	
	Date	Delay
A (%)	42	10
B (%)	58	17
C (%)	81	30
D (%)	81	30
Mean (%)	65	22
<i>N</i>	30	29

*Note.* Percent choosing the larger-later (*LL*) amount given different descriptions of time.

**Table 5** Experiment 4

Question	Description		
	Date	Date + Delay	Delay
A (%)	47	23	18
B (%)	50	33	25
C (%)	33	13	21
D (%)	30	13	11
Mean (%)	40	21	19
N	28	30	30

Note. Percent choosing the larger-later (LL) amount given different descriptions of time.

## Experiment 4

Experiments 1 through 3 demonstrated that the date/delay effect was robust under two different response modes and that it persisted in choices with monetary incentives. In Experiment 4, we investigated what happens when both time descriptions are available simultaneously by comparing the Date and Delay conditions with a combined *Date + Delay* condition. We were interested in whether one time description would dominate or whether both would contribute to an impression of temporal distance, such that the choices in the *Date + Delay* condition would be between those in the separate Date and Delay conditions.

### Method

Participants were 90 students from the LSE who were approached in the courtyard or library and asked to complete our questionnaire. They chose between pairs of outcomes in which the times were described as either Dates, Delays (in months), or as both (*Date + Delay*). The rewards were identical to those in Experiment 1, though the times differed (see Table 1). The format of the *Date + Delay* condition is shown below:

	Option 1	Option 2
You receive	£900	£1,200
When	in 4 months February 27, 2004	in 20 months June 24, 2003
Your choice	<input type="checkbox"/>	<input type="checkbox"/>

The Date and Delay questions were exactly like those used in Experiments 1 and 3.

As before, participants chose LL far more frequently in the Date than the Delay condition, with the *Date + Delay* condition in between but much closer to the Delay condition (see Table 5). Kruskal-Wallis tests revealed a significant overall effect ( $\chi^2(2) = 8.54$ ,  $p = 0.014$ ) and significant differences between the Date condition and Delay condition ( $\chi^2(1) = 6.7$ ,  $p = 0.01$ ) and between the Date condition and the *Date + Delay*

condition ( $\chi^2(1) = 5.6$ ,  $p = 0.018$ ), but no difference between the Delay condition and *Date + Delay* condition ( $\chi^2(1) = 0.086$ ,  $p = 0.769$ ). It appears that when both descriptions are presented concurrently, people choose as if they had seen the Delay description only.

## Experiment 5

Hyperbolic discounting refers to the hypothesis or observation that discount rates decrease as outcomes recede into the further future. This means that the rate at which the present value of a reward declines as it is delayed from  $t$  to  $t + 1$  is decreasing in  $t$ . Hyperbolic discounting predicts that for any pair of rewards separated by a fixed interval (i.e., holding the time separating  $t_{SS}$  and  $t_{LL}$  constant), delaying the onset of that interval ( $t_{SS}$ ) will increase the likelihood of choosing LL. In most tests of this prediction, time has been described as a delay and usually the prediction of hyperbolic discounting has been confirmed (e.g., Bleichrodt and Johannesson 2001; Kirby and Herrnstein 1995; Read and Roelofsma 2003, Experiment 1; Keren and Roelofsma 1995; Van der Pol and Cairns 2002—though see Ahlbrecht and Weber 1997; Baron 2000; Holcomb and Nelson 1992; Read 2001, Experiment 2).<sup>3</sup> On the other hand, in the few experiments describing time in terms of calendar dates, hyperbolic discounting has not been observed (Pender 1996; Read 2001; Read and Roelofsma 2003, Experiment 2). Thus, the existing literature suggests that hyperbolic discounting may occur only (or, at least, primarily) when time is described as a delay. However, the hypothesis has never been tested within a single study that compares date and delay descriptions, so it is unclear whether the differences between studies are due to time description, or to other differences between the methods used. To answer this question, we conducted Experiment 5.

### Method

The experiment used a computerized choice titration method described in earlier papers (Read 2001, Read and Roelofsma 2003). Participants chose between delayed outcomes presented on a computer screen as below:

Amount	£500	£1,000
When received	November 29, 2002	February 28, 2003

They indicated their preferred outcome by depressing one of two keys. Following the choice, either  $x_{LL}$  or  $x_{SS}$  was adjusted to bring the options closer together. For example, if £1,000 was chosen in the question above, the value of  $x_{SS}$  may be increased to £750 and the

<sup>3</sup> This list excludes many studies that failed to hold interval length constant.

**Table 6** Experiment 5

Interval length	Description	N	Interval endpoint				
			$t_1$	$t_2$	$t_3$	$t_4$	$t_5$
Short	Date	20	August 30, 2002	November 29, 2002	February 28, 2003	May 30, 2003	August 29, 2003
	Delay	29	in 1 month	in 4 months	in 7 months	in 10 months	in 13 months
Long	Date	19	October 25, 2002	August 29, 2003	April 30, 2004	January 28, 2005	October 28, 2005
	Delay	21	in 3 months	in 12 months	in 21 months	in 30 months	in 39 months

*Note.* Interval endpoints for all conditions. Interval 1 is from  $t_1 \rightarrow t_2$ , interval 2 from  $t_2 \rightarrow t_3$ , and so on.

respondent would choose again. This process was continued until an indifference point was reached—where  $x_{SS}$  at  $t_{SS}$  was equal in subjective value to  $x_{LL}$  at  $t_{LL}$ .

At the indifference point, the relationship between SS and LL can be expressed as  $x_{SS} = \delta^{(t_{LL}-t_{SS})} x_{LL}$ , where  $\delta$  is a discount factor that indexes the average value of 1 unit of value if it is delayed by 1 unit of time (years, in our study). Rearranging these terms, we derive the discount factor, our dependent variable, as follows:

$$\delta = (x_{SS}/x_{LL})^{1/(t_{LL}-t_{SS})}.$$

Participants were 89 students and staff from LSE. They were divided into four groups based on whether time was described as a Date or Delay, and on whether the discounting intervals were Long or Short. The stimuli are summarized in Table 6. Four discounting intervals were chosen that spanned either a 12-month (Short) or 36-month (Long) period. For example, in the Short condition, Interval 1 spanned the dates from August 30, 2002, to November 29, 2002 (1 month to 4 months), Interval 2 spanned the dates from November 29, 2002, to February 28, 2003 (4 months to 7 months), and so on. The four intervals appeared to respondents in random order, embedded in a series of other similar questions.<sup>4</sup>

## Results

The mean values of  $\delta$  are depicted in Figure 1. As can be seen, there was a strong date/delay effect, with  $\delta$  consistently higher when time was referred to as a date than when it was referred to as a delay  $F(1, 85) = 9.9, p < 0.002$ . Moreover, there was strong evidence of hyperbolic discounting in the delay, but not the date condition: Within-subject contrasts revealed a strong linear trend in the delay condition ( $F(1, 48) = 34.43, p < 0.0001$ ), but no linear trend in the date condition ( $F(1, 37) = 0.13$ ).

<sup>4</sup>These data are excerpted from a much larger study with many conditions, designed as a follow-up to Read (2001). We report a subset of these conditions to simplify exposition. What we report here, however, is consistent with every condition in the study. In particular, there was a strong date-delay effect in every comparison.

Consistent with our hypothesis, hyperbolic discounting is only observed when time is described in delay terms.<sup>5</sup> This demonstrates the important role that temporal description plays in determining the shape of the discount function.

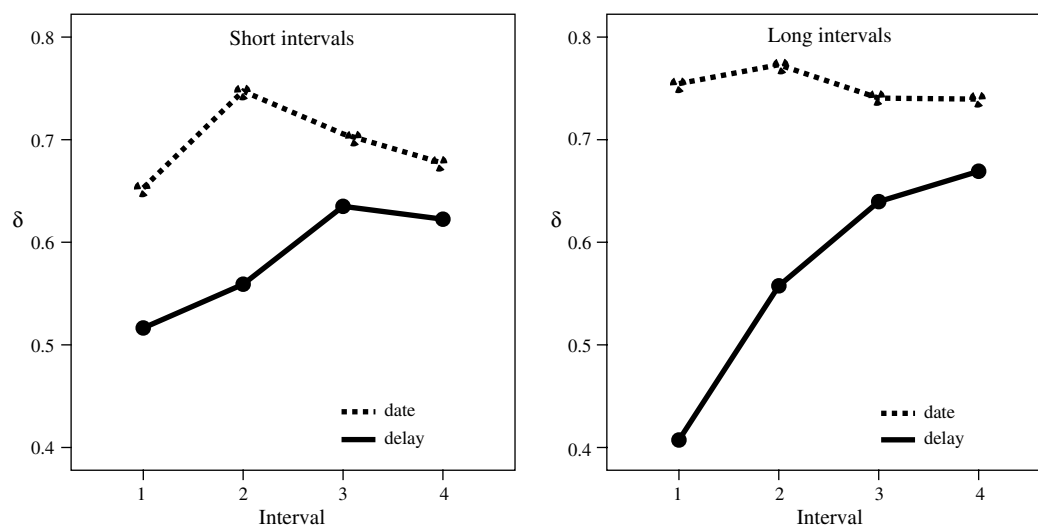
## Discussion

This paper provides evidence for a date/delay effect in intertemporal choice: People exhibit less discounting when time intervals are described as dates than when they are described as delays. Like other framing effects, the date/delay effect shows that when people encounter a specific problem description, they do not automatically translate it into a canonical representation that is shared by all formally identical descriptions. Rather, they appear to adopt the perspective they are presented with—and all its psychological concomitants—for whatever choice and judgment is at hand. That this occurs for dates and delays is particularly striking because these two temporal frames are extremely familiar and our respondents will have been repeatedly and recently exposed to both types of descriptions (e.g., “Let’s meet next week, same time” or “No I have a meeting then, but I’m free on the 23rd and the 25th”).<sup>6</sup> But experience with both descriptions does not mean that one description is effortlessly transformed into the other, or that the necessary effort will always be expended (unless some calculation, such as an interest payment, requires it). Indeed, the experience and familiarity with both frames may inhibit

<sup>5</sup>The term “hyperbolic discounting” is sometimes used to refer to quasi-hyperbolic discounting or present-biased preferences, which is the disposition to overvalue immediate outcomes relative to delayed ones (Laibson 1998, O’Donoghue and Rabin 2001). Because all outcomes in Study 5 were delayed by at least a month, it cannot be considered a test of this version of hyperbolic discounting.

<sup>6</sup>In this respect, the date/delay effect appears even more striking than many other framing effects because people rarely have a comparable opportunity to recognize the correspondence between frames. For example, though experimenters can describe formally equivalent gambles in different ways by partitioning an event into its mutually exclusive components, it would be rare for a gambler at the roulette wheel to think of his bet on “red” as a bet on the union of the 18 red numbers {2, 27, etc.}, each of which has a probability 1/38 of occurring.

Figure 1 Experiment 5



Notes. Values of  $\delta$  measured for intervals of the same length but varying in onset, under both date and delay descriptions. Interval 1 ranges from  $t_1 \rightarrow t_2$ , interval 2 from  $t_2 \rightarrow t_3$ , and so on.

motivation to transform one into the other. Respondents are not like Americans landing in England who *must* do a calculation to decide what to wear at 20° Celsius. Rather, whether given a date or delay frame, they likely have some intuitive impression of how long the period in question is and can make the required judgment or choice without feeling any need to consult an alternate temporal metric. What is interesting, of course, is that the intuitive impressions of temporal length associated with the two descriptions may differ.

Although the existence of a date/delay effect is clear, we have not isolated its psychological bases. In the next section, we consider several possible explanations and discuss the degree to which each is consistent with our experimental data. While none of these hypotheses can neatly account for all our findings, all of them correspond to plausible psychological processes and indeed may all play a role in producing the date/delay effect.

#### What Is Responsible for the Date/Delay Effect?

Our starting point, based on our reading of Strotz (1955), could be called the *attention-focusing* hypothesis. According to this account, temporal framing influences the attention allocated to the value versus the timing of a future outcome, with value receiving relatively more attention under the date than under the delay description. Correspondingly, to the extent that outcome value is emphasized, larger rewards will be more preferred despite being received later. This attention-focusing hypothesis is consistent with the basic date/delay effect as found in our choice studies whereby the attractiveness of LL increased in the date frame (Experiments 1, 3, and 4).

A second explanation is the *choice strategy* hypothesis, whereby different temporal frames facilitate or impede different decision strategies. The delay condition may encourage a compensatory strategy because delays and amounts are both continuous numeric variables, denominated in integers, which may encourage respondents to establish some exchange rate to guide their evaluation of, and choice between, outcomes. However, the date description impedes such a process. Participants must either transform dates into corresponding delays or adopt a noncompensatory decision strategy, such as always choosing the option that is better on the more important attribute (e.g., Slovic 1975, Payne et al. 1993). If amount is the “more important” attribute—the one to which respondents are likely to defer when deciding between options that differ on both dimensions—it would explain the preponderance of LL choices in the date conditions. This hypothesis is consistent with the choice data from Experiments 1, 3, and 4. However, as with the attention-focusing hypothesis, the choice strategy hypothesis cannot account for the persistence of the date/delay effect in the matching task, which effectively forbids the application of a lexicographic strategy (see Tversky et al. 1988, Frederick and Shafir 2005).

A third explanation, the *preference for precision* hypothesis, is that people prefer the precision of a date to the vagueness of a delay, and so are more patient over a dated interval because they are more certain that the larger-later reward will actually be received. There will certainly be circumstances when dates do make us more secure than delays. For instance, the promise that we will be paid “on August 3” likely inspires more confidence than the promise that we will be paid “in two months.”



Again, while this could partly explain the phenomenon, it falls short of being sufficient. First, the arguably more ambiguous temporal referents in the delay condition apply to *both* future outcomes (SS and LL). Thus, if both outcomes are discounted by some additional constant factor (e.g., “Unless they give me a date, I’m only 80% certain I’ll get it at all”), it would not shift preference in favor of the smaller-sooner uncertain reward.<sup>7</sup> Second, although Months is clearly a less precise temporal description than Weeks, the larger delayed reward (LL) was chosen *more* frequently in the Month condition in Experiment 1. Finally, in the Date + Delay condition of Experiment 4, which does specify a future date, people were just as impatient as in a delay-only condition. It seems unlikely that people believe that “in six months, on July 3, 2005,” is a less definite promise than “on July 3, 2005.”

A fourth account is the *differential time estimation* hypothesis which posits that relative to the delay interval, the date framing causes the temporal interval to be underestimated. We do not merely mean that it diminishes the *impact* of temporal distance—that is the phenomenon for which an explanation is being sought—but that people actually underestimate the temporal separation in terms of some other metric (e.g., they don’t *realize* that the time between the “end of August” and the “beginning of December” spans 13 weeks). This hypothesis can account for the results of Experiments 1, 3, and 4. Moreover, unlike the first two hypotheses, it can account for the matching results of Experiment 2 because perceived delay should affect all of the matching responses regardless of whether time or amount is the response variable.

A fifth hypothesis, related to the differential time estimation hypothesis, is the *similarity* hypothesis based on recent work by Rubinstein (2003) and Leland (2002). They argue that discounting diminishes as a function of the judged similarity of the time-points marking the beginning and the end of an interval. According to this account, the date/delay effect occurs because two dates seem more similar than two delays spanning the same time period. For example, in question D from Experiment 1, respondents evaluated two delayed options framed either as 3 months versus 16 months, or as August 29, 2003, versus September 24, 2004. By any metric of similarity, 3 and 16 months seem pretty different, perhaps because the ratio of the two time periods (which exceeds 5:1) is so salient. Thus, we should expect (and we observe) high discounting (low values of  $\delta$ ). Conversely, the corresponding dates do not seem so different: 29 is similar to 24, 2003 is similar to 2004, and August is adjacent to September in the calendar. Rubinstein argues

that hyperbolic discounting occurs because the similarity between two time-points separated by a common interval increases with the onset of that interval: 12 months is more similar to 11 months than 2 months is to 1 month. Similarly, Prelec and Loewenstein (1991) suggest that similarity between two numbers depends on both differences and ratios, and that discount rates decrease with time because the ratio of two delays sharing a common difference diminishes when a constant delay is added to both. Our data are consistent with such accounts because a similar logic does *not* apply to dates: The similarity between dates separated by a common interval does not change with onset time, e.g., the similarity of August 13, 2005, and September 22, 2005, is no greater than the similarity of August 13, 2004, to September 22, 2004. Thus, we have hyperbolic discounting for delays but not for dates.

The last two hypotheses not only predict differences in the degree to which future rewards are valued (the focus of our experiments, and indeed nearly *all* experiments on discounting); they also imply that temporal framing will affect all sorts of temporal judgments that have nothing to do with valuation. As a casual “test” of this, we told a group of MIT students on December 8 that a baby boy who is 12 weeks old today currently weighs 13 pounds. We then asked them to guess how much that baby boy would weigh at a future time, manipulating how the future time was described. In the delay group, we framed it as “when he is 36 weeks old” whereas in the date group we framed it as “on May 5.” To the extent that the future date seems more similar (or closer) to the present in the date frame, the baby should be perceived to have less time to grow and, thus, should be judged to be smaller at the specified future time. That is, indeed, what we observed: Respondents in the Date group estimated that the baby would attain a weight of “only” 24.7 pounds, compared to those in the Delay group who thought he would weigh 29.3 pounds,  $t(59) = 2.013$ ;  $p < 0.05$ . (Both of these are overestimates; the actual expected weight is roughly 21 pounds.)

The baby data illustrate and foreshadow the difficulty of precisely specifying the underlying psychology of the date/delay effect. Though the data could be cited in support of either the *differential time estimation* hypothesis or the *similarity* hypothesis, if similarity refers to temporal distance, it is unclear whether one can distinguish the hypothesis that the baby is judged to be smaller in the delay frame because the future date seems more “similar” from the hypothesis that he is judged to be smaller because the time interval is seen as being objectively shorter.<sup>8</sup> Moreover, the baby

<sup>7</sup> One might, however, posit that people apply higher discount rates whenever the future is referred to in more ambiguous terms.

<sup>8</sup> Moreover, estimates of time can only be made in terms of some other metric of time. Thus, it is difficult to test whether time is underestimated when described as dates relative to delays, without

data are also consistent with the *choice strategy* hypothesis. In the delay condition, respondents may reason that since 36 weeks is 3 times as long as 12 weeks, the baby may be 3 times bigger by then (39 pounds). Even if they reject this computation, it may act as an anchor for their ultimate response. Again, however, it is difficult to determine whether someone in the delay condition who gave a response such as “31 pounds” did so because they insufficiently adjusted from the 39 pound anchor, because 36 weeks and 12 weeks sound “dissimilar,” or because the corresponding dates sound less than 24 weeks apart. We leave the resolution of these questions to future research. Our primary goal in this paper was to document the date/delay effect (which we did in Experiments 1 through 5), to speculate about its causes (which we have just done), and to sketch out some of its implications (which we do next).

### Implications

The date/delay effect has practical implications. One lesson is that commercial retailers should refer to future outcomes in terms of calendar dates if they want to discourage discounting (e.g., if the buyer must incur unavoidable long shipping delays), but in terms of delays when they want to encourage it (e.g., when the seller makes money from express shipping). The implications may be greatest in the domain of investment and credit offerings. When people think of the future in terms of calendar dates, they will be more likely to invest and less likely to borrow, because there will be less discounting over the time that elapses before future returns will be received or future payments demanded. For instance, when offering bonds, Euro bills, Treasury bills, and other fixed-term securities, it would be better to emphasize the specific listed calendar date on which they mature, as this should reduce discounting and increase willingness to invest at a given rate. Conversely, it may be best to offer loans by referring to the delay until the loan comes due, as this should make future payments seem more distant and therefore less onerous. In fact, bonds and loans *are* advertised this way, although we doubt this is a strategic decision.<sup>9</sup> Another implication is that

“buy now, pay later” schemes should be more attractive when described in delay terms (“pay nothing for six months”) than when described using specific dates (“pay nothing until June 2005”). An informal survey of retailers in the UK making such offers found that most do describe their offerings in this way (though again we doubt this is strategic).

Governments could employ the date/delay effect to convey the attractiveness of savings and the unattractiveness of debt. It may be more effective to advertise savings by marketing it as “if you put £1,000 in a cash ISA [Individual Savings Account] on the first day of 2005, it will be worth £1,500 by Christmas 2015” than by marketing it as “if you put £1,000 in a cash ISA today, it will be worth £1,500 in less than 11 years.” Likewise, to decrease consumers’ willingness to incur debt, government regulation could require merchants to specify in calendar terms the implications of loans, as in “...on January 28, 2005, February 27, 2005, and the last Friday of every month thereafter until December 29, 2009, you must pay Johnson’s Electronics £100.” Correspondingly, annuity products might seem more attractive when payments are described as being received on a specific date each year.

### Conclusion

Politicians and poets know that the quality and intensity of our responses to events is affected by *how* they are described. It should not surprise us, therefore, that the description of a temporal interval affects discounting. Moreover, this finding is unlikely to be limited to the contrast between date and delay descriptions per se. For instance, dates that are “special” may be treated differently than other dates, particularly if the specialness is directly specified (e.g., “Christmas 2015” or “on your next birthday”). Furthermore, there are still other ways of referring to time and each may be characterized by its own way of discounting the future. One of us has noticed that our students seem more panicked when told that they have 600 seconds left to complete their exam than when they are told they have 10 minutes left. Another of us, who is 37, has noticed that an outcome occurring in 15 years feels relatively imminent, whereas one occurring when he is 50 feels like the far distant future. Thus, temporal events indexed by a person’s future age could be discounted differently than those indexed in other ways. Perhaps individual differences in discounting can even be explained in part by the perspective that

directly confronting respondents with an equivalence judgment that effectively guarantees the detection of the identity of the two descriptions. That is, if respondents are asked, “How many weeks are between December 8 and May 5?” it seems likely that most will compute the correct answer, and that any errors will have no directional bias. A bias might be detected if respondents were forced to quickly respond, but it would be unsatisfying to use such a finding to explain the bias for deliberate judgments like those made in our experiments.

<sup>9</sup> There are good reasons for the two kinds of presentation that have nothing to do with the date/delay effect. A Euro bill offering is put on the market on one day, and matures on a specific day,

and so it is natural—and legally required—to describe the offering in terms of dates. On the other hand, consumer loans are flexible instruments that vary both in the time when they become available, and the repayment term. Thus, it is natural to describe their temporal details in more abstract “delay” terms.

individuals tend to spontaneously adopt when thinking of the future, when no specific perspective is deliberately cued.

As a final note, our findings also serve as a caution to researchers. As discussed earlier, researchers usually choose temporal descriptions for arbitrary or pragmatic reasons. Yet they will then propose as general truths findings that apply only to the particular descriptions they arbitrarily or pragmatically chose—a possibility highlighted by Experiment 5, which shows hyperbolic discounting for one description but not the other. Perhaps researchers should more seriously consider the “operationalist” approach first advocated by Percy Bridgman in 1927 (almost 80 years ago):

If we have more than one set of operations [i.e., ways of measuring a phenomenon], we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations. (p. 10)

In intertemporal choice, as in any other area of judgment and decision making, if different response modes or superficially different descriptions yield different preferences (discount rates, risk preferences, attribute weights, etc.), we should be careful to delineate how broadly (or narrowly) a phenomenon applies—whether it is universal or whether it applies only to the specific set of descriptions or measurement operations we have chosen. Date discounting and delay discounting are different concepts, and the implications we draw from one kind of discounting do not necessarily apply to the other.<sup>10</sup>

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<sup>10</sup> Bridgman later argues, “The practical justification for retaining the same name [for a concept measured via different operations] is that within our present experimental limits numerical difference between the results of the two sorts of operations has not been detected” (p. 16). This justification does not, of course, pertain to the respective results of our alternate temporal framings, where the differences are not only detectable but quite considerable.

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